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SHORTER ARTICLES AND DISCUSSION

ON PRACTICAL VITALISM

IN a series of critical and polemical essays, published during the past few years in American journals by diverse authors, particularly by Jennings, the problem of vitalism has been discussed in a manner that may seem exhaustive.

There would appear to be no possibility of adducing new arguments in the matter. If in spite of this a new presentation is here attempted, it is because the author holds a standpoint entirely divergent from what has been thus far set forth in the discussion.

If it is true that the argumentation of the promoter and leader of the new scientific vitalism—Driesch—becomes at times somewhat metaphysical, it appears to me also that the criticism, as made by Jennings, tends at times to become dialectical and sophistical.

I can not otherwise characterize the tendency to efface any specific difference between the living and the non-living. By isolating at random a feature of the living and comparing it with an inorganic model one can indeed seem to show the identity of the two. But in this procedure we recognize the typical method of the ancient sophists. I can find nothing of interest, for example, in such an argumentation as the one cited below from Jennings.[1]

In a rejoinder to Lovejoy, who insists "that the same phenomena occur in a given organism in spite of profound modifications of the composition and configuration of the parts" Jennings objects that we have here

> a proposition that holds for things in general. An iron body of a certain form moves toward the earth. We may change the form in most varied ways . . . change the material, substitute lead, brass, stone . . .; it still moves toward the earth.[2]

Nothing is easier than to prove that black and white, plant and animal, man and monkey, are "fundamentally the same."

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[1] A typical one for the antivitalistic criticism.

[2] AMERICAN NATURALIST, 1913, p. 395.

But does an affirmation of this sort annihilate in any way the specific difference between man and monkey, or diminish the interest of science in this specificity?

The innumerable attempts of the critics of vitalism to prove by comparison of certain isolated features that the living is nothing more than an extreme complication of the non-living fail, because the analysis in such cases is never exhaustive. One may prove that living and inorganic coincide in many points; he can not prove more.

I do not see why these points of coincidence are of more importance and interest for our conception of the matter than the points of undeniably distinctive difference, even though the latter are as yet unanalyzed.

The best way to test the validity of an idea or hypothesis is to follow it to its most extreme but logically inevitable consequences, taking these as a statement of the proposition involved.

If we follow this method in order to obtain an objective and exact formulation of the essence of vitalism (or of its antithesis, mechanism), we can say that what mechanism asserts is this: Whenever a certain configuration of matter occurs or is given, there also what we call "life" is found; or in more popular terms, the artificial production of a living organism from "non-living" matter would theoretically be possible.

Vitalism, on the other hand, is a standpoint that in last instance denies such a possibility.

It is clear that both the assertion and the negation are unprovable, and as such are matters of faith, not of emprical science.

If one attempts to give an estimate of the two from the standpoint of science, sympathy must, it appears to me, incline to the vitalistic view, since scepticism is the very palladium of exact science.[3]

It is generally overlooked that if one of the two opponents is to be reproved as aggressive, that one is the mechanist rather than the vitalist. The mechanist in asserting that he knows more than can be proved is filled with a scientific optimism of a somewhat frivolous character.

Yet it is the moderate agnostic standpoint, declaring no belief in the possibility of artificial synthesis of the living so long as that is not proved, that is subjected to ridicule as a dogmatic, obscurantist and non-scientific doctrine.[4]

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[3] It may be objected that a negation is dogmatic to the same degree as an assertion. This may be true. But one can replace the term "negation" by some other less radical expression, such as "doubt," without altering the essence of the standpoint.

The entire problem to me falls in the domain of ''Natur-philosophie,'' that branch of our knowledge which can not directly prove the truth or logical necessity of the results of investigations made in its field; can do no more than to make them plausible; and thus give to us a genuine sensation of mental satisfaction.

There is no intention here of participating in the endless dispute above sketched; I do not know what could be added in this direction, from a vitalistic point of view, to the formulations of Driesch.[5] Our purpose is the defense of the right to a *practical vitalism,* as a method of exact empirical (although not necessarily experimental) investigation.

We do not care whether the methods demanded by such a vitalism are or can be proper also for inorganic investigation.

It appears that they are not, for the mechanists oppose their ''veto'' in the name of exact science to all constructions of the vitalistic system, even though not fully analogous with that which will be detailed below.

Practical vitalism claims the right to be restricted in formulating hypotheses only by postulates of logic and of the general theory of knowledge, and by nothing else.

[4] The same point can be made with regard to the other aspect of vitalism —the so-called ''experimental indeterminism.'' As to this, it must be admitted that the empirical evidence seems to favor the vitalistic standpoint. The assertion of the mechanists, that experimental indeterminism can not hold for the living, is likewise a matter of faith, and the burden of proof falls upon those who make it.

[5] I must nevertheless confess, despite my profound admiration for Driesch's work, that I find that his chief experimental foundation of vitalism, by means of his masterly analysis of certain cases of regulation, fails to produce the desired effect; chiefly because the entire argument rests upon certain experiments that are, as one may say, a lucky chance in biological investigation. It would be quite possible that no organisms having the marvelous powers of regulation and equipotentiality shown by Tubularia, the sea-urchin or *Clavellina,* should ever be discovered. Can it be admitted that a scientific proof of vitalism as the basis of biological research would therefore remain inaccessible? The argument in such a capital problem must, I think, rest on a more general basis, one resulting from an adequate analysis of essential and genuine vital phenomena. I incline therefore to consider Driesch's further analysis, as presented in his ''Science and Philosophy of the Organism'' as a no less valuable part of his work.

We hold as a justified demand of the theory of knowledge that every hypothesis must be fruitful; that is, it must give a number of deductions that can be verified empirically. Every hypothesis which permits us a prediction is to be considered a step in the progress of knowledge, until such time as it is replaced by a new one, more suitable or more fruitful.

Biological, and particularly embryological, investigation needs sometimes to introduce as a hypothesis for the explanation of certain empirical facts the idea of so-called "immaterial" (or in Jennings's terminology "non-perceptual") factors.

This is the chief point on which are based the recriminations of most critics of vitalism, especially of Driesch's vitalism.

The belief in such "non-perceptual" factors is in Jennings's mind synonymous with obscurantism or dogmatism. To Ritter "the vitalism . . . is the belief that organic phenomena can not be fully explained by referring them to the material elements of which organisms are composed, but that something *not really belonging to the natural order* [?] . . . is present in living things" (italics mine).

To me it is entirely obscure why the term "non-perceptual factor," employed by Jennings in a logical and consistent manner, is by him rejected as nonsense.

His formulation of the non-perceptual is very clear.

> Conditions subject to diverse physical tests will here be called either perceptual or physical.[6]
>
> A non-perceptual agent would be one which though producing at a particular time a particular physical event, was not subject to other physical tests for its presence.[7]

I have given a formulation much resembling this, of what I call the "immaterial factor" in my paper bearing that title, printed in the "Festschrift für Schwalbe."

This work was to have been published the first of August, 1914, at the very moment of the outbreak of the war. Whether it has been issued I do not know.

My definition is as follows:

> Als materiell gilt uns im allgemeinen ein Objekt unserer Erkenntniss welches eine Mehrzahl von einander unabhängiger Eigenschaften (sc. Wirkungsweisen) in sich vereinigt und sowohl in Tätigkeit als in Ruhe befindlich wenigstens gedacht werden kann.

[6] Johns Hopkins University Circular, 1914, No. 10, p. 8.

[7] *Ibid.*, p. 12.

Ein zur Erklärung bestimmter Wahrnehmungen ersonnener Factor von dem eine derartige Annahme d. h. ein Zustand der Nichtbetätigung widersinnig wäre mag folgerichtig als "nicht materiell" bezeichnet werden.

Neither Jennings's "non-perceptual" or my "immaterial" can be considered an illogical or contradictory conception. Criticism must, so far as my own doctrine is concerned, be therefore concentrated solely on the strength of the empirical foundation for the hypothesis of immaterial factors in any given case.

Besides the logical definition given above, an examination is required of the question; What exactly can be meant by, or *how can one be led to assume, an "immaterial factor" as a result of experimental investigation, or at least as a hypothesis impelled by such a result?*

To Jennings the assumption of a "non-perceptual agent" leads directly to, or is synonymous with, the so-called "experimental indeterminism," as admitted by Driesch.

He seems to neglect every other possibility of the action of an "immaterial factor." I do not see that this is inevitable.

To me the essential point of the problem lies in the question of the "bearers" for any sort of empirically detectible action (induction, force, or the like).

Suppose that it were found that the factors directing the movements of a given element of a living organism (for example, the cell of an embryo), in a given direction m to the point n, lie outside itself.

We will then assume at the point n a center of forces.

Suppose now that we can deduce from this assumption certain consequences that will be subsequently verified empirically.[8] Our assumption that gives us possibilities of prediction becomes then a scientific reality. We say "reality," although it may remain somewhat hypothetical. We find the same condition of affairs in the imperceptible but strongly inferred realities of physics, etc.

[8] I find that there is a point at which Jennings's conception of the "non-perceptual" seems to lead us wrong. It is well to say with Jennings that such an agent is one producing at a particular time a particular physical event *but not subject to other physical tests for its presence* (italics mine). But Jennings seems not to take into consideration that a "particular physical event" or a "single mode of action" (in my formulation) can lead to many empirically verifiable *consequences.*

Suppose now that at the point of space where we have projected the center of forces there lies some element of the embryo, such as a cell. The scientific routine will call this element the "bearer" of the forces in question.

But it is also possible that no element and no matter is to be found at this point.

The first impulse will be to search for some other element of the embryo, situated elsewhere, that can act as such a center, by irradiating certain "lines of force," which influence in some manner the movements of the first considered element. We will, however, assume a case where no such element acting at a distance can reasonably be supposed. What now?

If the fundamental assumption holds true, that the factors determining the movements of the element lie outside of itself, we find ourselves confronted by the following alternative:

Either the presumed factors have a bearer that is not cognizable, or they have no material bearer at all!

It is clear that if we deny the existence of bearers that are evidently perceptible, we can also exclude the possibility that such bearers exist, but are invisible owing to their minuteness; for the presumed center of forces lies according to our assumption outside the organism; or in a district of it where there is no formed embryonic matter at all.

Thus under the circumstances our two alternatives signify the same thing, for to say that there is a bearer of factors that is cognizable solely as the factors themselves involves a tautology; an assumption of the sort so well characterized by the French as a "hypothèse gratuite." While any one is free to make such an assumption, no scientific use can be made of it. Methodologically it is perhaps comparable to Kant's "Ding an sich," which likewise must remain without empirical content.

As a fundamental postulate of biological (and especially of embryological) research, there can therefore arise the conception of factors which, although spatial and localized in space, have no material bearers, and as such may be denominated immaterial.

Is such an idea indeed nonsense; something that proves the obscurantism of its promoter?

I am well aware that the "immaterial" factor here presented is far from coinciding with Driesch's Entelechy or with any analogous agent that is *e definitione* not solely immaterial, but also non-spatial.

If the entire weight of antivitalistic criticism is directed and concentrated wholly against such ideas as that of Entelechy; and if the mechanist will agree with me that a spatial localization of a center of forces may be assumed without necessarily combining this with a material bearer, I shall be much gratified. But I fear that this is not the case. The "dynamical præformation of the morphe," as I have elsewhere called the immaterial but spatial factors of morphogenesis,[9] must, I fear, fall under the same anathema as the classical vitalism.

To resume the chief postulate of my own "vitalism": if morphogenetic investigation is led in a rigorous inductive way to assume a spatial factor at a definite point inside or outside the embryo, no difficulty or contradiction or nonsense arises if no "embryonic" matter, or what is the same, no material "bearer" for this factor can be found at that point. Yet of course no one can be prohibited from forming any sort of hypothesis as to such functionless bearers. It may be even a psychological necessity to form such hypothesis, for we love a "Ding an sich." But such will form no part of empirical research.

The right to work with such immaterial factors, and in the inductive way set forth above, is, for me, the essence of *practical* vitalism.

We have now to examine consequences and postulates derived from our fundamental assumption, which seem to present very great difficulty. If we admit a dynamical factor localized in space but not derived from a material bearer, it will be asked, whence comes and how arises this factor?

The question of causation is based on a postulate of knowledge that can not be eluded; it must be answered in some manner.

I will attempt to point out briefly how one can think the origin or evolution of such an immaterial morphogenetic factor, although it must be insisted that we have here a problem which does not stand in immediate connection with the purely empirical method of investigating the factors considered, so to say, *per se* in their activity.

I see no difficulty in assuming an immaterial causality; that is, the arising of an immaterial factor having a certain property

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[9] *Biologisches Centrablatt*, Bd. 32; *Archiv. f. Entwicklungsmechanik*, Bd. 39; *Festschrift für Schwalbe*, 1914.

(for example, configuration) from another less complicated immaterial factor, and so on.

The chain of immaterial factors could in this manner logically be pursued backward to the beginning of the embryogenesis, or to the egg.

As to the relation of such immaterial factors to Driesch's entelechy, they can be ranged solely in the category of "means" (Mittel) of the latter for the purpose of morphogenesis.

But I repeat that this is for me a matter belonging for the present not to experimental investigation, but to the domain of "Naturphilosophie."

If it appears as if I agree in this point with the "standpoint of radically experimental analysis" of Jennings, this is not really the case. The latter author seems to reject all that does not belong to experimental investigation.

I think, on the contrary, that vigorously logical considerations, deductive and even inductive, on the given empirical data form a legitimate and integral part of our science of nature.

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